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disrupted many ancient traditions and institutions. "Integrate the evidence," by all means. That is a scientific course; but before you "accept the results that eventuate" take care that you have not overlooked important though possibly awkward, facts, and that you have given due weight to every argument.

With regard to any purely incidental allusion to the case of the Sia, as Dr. Kroeber puts the matter it would seem as if I had invented the explanation of their violation of their laws of exogamy, whereas I simply quoted Mrs. Stevenson's account. Does Dr. Kroeber deny it? I did not refer to the case of the Zufi because their organization was not in dispute—not because, as he sarcastically suggests, "the case is too lacking in significance to refute or mention." It is significant; but I am sure that its significance is exactly such as would be convenient to the advocates of the priority of patrilineal institutions.

Finally, I am happy to concur with Dr. Kroeber in his disclaimer of nationalistic aims. The field of science and scientific history is, as he says, international. Science knows no national boundaries. Least of all could I, who am so largely indebted to the anthropologists of the United States, be guilty of any nationalistic aims; and I am equally sure that Dr. Kroeber on his side needs not to plead for acquittal. What both of us desire as the result of this friendly encounter is the advancement of scientific truth, and nothing else.

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COMMENTS ON THE ABOVE

I GRATEFULLY acknowledge Mr. Hartland's sustained courtesy under provocation. We appear to differ chiefly in the degree of objectivity which we accord to isolated "facts" when they collide with correlations. Biologists control a mass of integrated data which show that in general insects begin their careers as eggs and pass through a more or less larval and often pupate form before they reach the final or adult stage. If a newly discovered butterfly is known only in the adult form, we do not therefore conclude that the species lacks the preceding forms. In fact, should it be asserted that any given species had been observed to change from imago to larva, the observation would be doubted, not accepted as a "fact," on the ground of being contrary to all known cognate data. The observation would have to be repeated on the full life history of the species in question, and under every safeguard against error, before serious attention could be granted it.

Now, what corresponds to this complete life history of an insect, in the issue between Mr. Hartland and myself, is the history of an individual tribe over a long enough period for it to undergo a pretty profound modification of its institutions. Such a time would normally be a much longer period than that during which we have even known any American tribe to exist; to which it may be added that all we ordinarily have of such histories is a paragraph by an explorer of a century or two ago, some subsequent notices by travelers or soldiers of a literary bent, and perhaps a more or less intensive study by one or more subsequent ethnologists. All this is not Mr. Hartland's fault; but it leaves his case much in the status of that of the insect observer who saw a butterfly in the box in the evening, a caterpillar in its place in the morning—and then his wife came along and threw away the box without any record having been made of its chinks—nor in fact whether the lid was not left open over night. The kind of intimate, reliable, institutional history of the individual North American tribes that Mr. Hartland could properly base his case on, simply does not exist. It probably never will be recovered in the necessary fulness. We must therefore fall back on inferential probabilities based on averaged experience. This I perhaps insist on doing to an exaggerated degree; but he seems to me to do to an insufficient degree, so far as the North American matrilineate is concerned. If anyone reported that the normal stature of men in Pike county, Missouri, was seven feet, I should not hesitate to rule such a "fact" out of the realm of belief, even though six observers alleged it and the occurrence does not transcend known physiological possibility. I should not even trouble to sift the reported evidence in detail, much less make a trip to Missouri with a measuring rod. I do not wish to suggest that Mr. Hartland's attitude is as gross as this comparison might imply. I do not even charge him with unreasonable naïveté. But I must ask to be excused from discussing evidence as fragmentary and miscellaneous as that on which he relies, so long as a putting together of all the really linkable evidence points to opposite conclusions.

Mr. Hartland brings forward without pressing very far one argument that I admit to be strong. I have appealed over the heads of isolated bits of evidence to the findings of the North American evidence as a whole. He in turn is correct in appealing from this to the evidence of the whole human history. But of course no mere count of souls, nationalities, or periods suffices. Just as mammals are more numerous, more developed, and more generally important than the tunicates, but the latter or the still smaller group containing the amphioxus are of

equal significance for an understanding of the course of chordate evolution, so native American culture must be equated with almost the whole of Old World culture. Each is a coordinate unit, essentially self-developing, but necessarily to be taken as a unit because all its parts or members have for a long time past stood in more or less close connection with other parts thereof. The only other such unit of whose distinctness we are at all sure is Australia. Even therefore if both Eurasia-Africa-Oceania and Australia proved demonstrably to have changed from matrilinear to patrilinear descent, the case would stand only two to one against aboriginal America; which is not sufficiently overwhelming to compel a reconsideration of the latter field. Such a result might indicate that there existed a preponderant inclination for "paternal descent and progress in civilization" to be associated; it would not establish the "organic connection" between them of which Mr. Hartland speaks, and which appears to be the emotionally colored origin of his attitude, just as it is the irritating stimulus against which I inevitably react.

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"MORE LIGHT:" A REJOINDER

A SECOND reading of Professor Dixon's remarks (*American Anthropologist*, vol. 20, p. 124) leads me to the conclusion that we are in almost complete, though latent, agreement. Professor Dixon gathers from my two reviews of his book (1) that I reject the influence of migration and diffusion on the development of Polynesian culture; (2) that I regard the evolutionary type of cosmogony as older than the creative, wrongfully imputing to Professor Dixon the same view; (3) that "explicitly for New Zealand, and implicitly for the whole of Polynesia" I dogmatically assume a homogeneous population. It will be relatively easy to dispel these misunderstandings.

1. I emphatically believe that the principles of culture contact and migration are as applicable in the Polynesian as in every other field of ethnology. But it is not necessary to apply these principles in each and every case.

2. I regard the *creative* as older than the evolutionary type of cosmogony. In a sentence of the *New Republic* review the two adjectives are interchanged. I am heartily sorry for this error, but precisely because it is so glaring, very few readers, I hope, were misled as to my intended meaning, which I think is manifest from the context.

3. My statements are quite dissociated from any belief in either the homogeneity or heterogeneity of the Polynesians.